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Veteran Status and Civilian Earnings

Dennis N. De Tray

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PREFACE

This report was prepared as part of Rand's DoD Training and Manpower Management Program, sponsored by the Human Resources Research Office of the Defense Advanced Research Projects Agency (ARPA). The purpose of this research program is to develop broad strategies and specific solutions for dealing with present and future military manpower problems, including the development of new methods for examining broad classes of manpower problems and specific problem-oriented research.

This particular research is concerned with the causes of differences in civilian earnings between veterans and men with no military service experience. When such background characteristics as years of schooling, age, and residential location are controlled for, veterans receive higher market wages, on average, than do men who have never served in the military. This report analyzes the causes of this "veteran premium" and assesses the fraction of that premium due directly to military service and the fraction related to factors that are correlated with, but not directly caused by, military service.

SUMMARY

What are the costs and benefits to a young man of joining the armed forces for a single tour of duty? Are there benefits to military service that transfer to the civilian sector? If so, how do such transfers affect the net flow of benefits between military and civilian sectors?

The answers to these and related questions have an important bearing on decisions that are now being made and will be made in the future concerning the nature of the U.S. armed forces. This report investigates one relationship between military service and post-service life: the influence of military service on post-service civilian wages. Young people at the beginning of their careers may not enlist in the military if they perceive that enlistment will result in lower future civilian wages; conversely, young people are likely to enlist in increasing numbers if military service is shown to enhance future civilian earnings prospects.

When one controls for such characteristics as age and education, veterans earn more than nonveterans in the civilian labor force. This "veteran premium" may be as high as 10 percent; were it attributable to service in the armed forces, it could represent a significant inducement to young men to enlist for a minimum tour of duty. Unfortunately, not all explanations for this premium carry the implication that it can be attributed solely or even perhaps principally to actual service in the armed forces.

Among the competing explanations for the veteran premium, two stand out: (1) the "filters" through which young men must pass to serve in the military, including physical and mental tests at induction, and a "certification effect" of having honorably served; and (2) training received either while in the military or through the G.I. Bill. Either or both of these factors could result in a positive differential between earnings of veterans and nonveterans when other work-related characteristics are controlled for; but only the second implies unambiguously that service in the armed forces *causes* veteran/nonveteran earnings differentials.

Section III of this report analyzes a recent micro-data set containing information on schooling, training, and work histories for about 5300 young men (the National Longitudinal Survey of Young Men, 1966-1975). This survey permits an improved analysis of earnings differences, especially among veterans who received training in the military. Two facts emerge from a multivariate analysis of these data. The first is that young veterans do indeed earn higher wages than young men who are not veterans, even when education, vocational training, actual work experience, number of years with current employer, age, and several regional and location variables are controlled for.

Second, veterans, especially veterans who receive training in the military, do not represent a cross-section of the population at large (all young men). Statistical results must be interpreted in light of the possibility of innate and unobserved differences in wage-related characteristics among veterans, veterans with training, and nonveterans. Assessment of the importance of these innate differences must await the release of the final panel of the National Longitudinal Survey (taken in 1976); preliminary indication of the probable outcome of such a study can be obtained, however, with currently available data.

One crude control for innate differences among sample observations is to look at wage changes over time—for example, by regressing 1975 wages on individual characteristics and each observation's 1971 wage. Results from this exercise suggest than when innate productivity differences among trained veterans, other veterans, and nonveterans are appropriately accounted for, training received in the military improves civilian productivity.

The fourth section of this report is concerned principally with the argument that wage differentials between veterans and nonveterans result from a "certification" effect. This effect could arise either because veterans must pass minimum mental and physical standards to

serve, or because they served successfully for at least one tour of duty in the military. In this regard, the issue is not whether veterans are more productive than nonveterans, but whether service in the armed forces signals above-average productivity to potential civilian employers.

Distinguishing between hypotheses generated from certification or signalling theories and hypotheses that arise from a human capital earnings model is difficult, as the two theories often predict similar outcomes. Several distinct hypotheses do arise, however, for wage changes over time, and the 1960 and 1970 U.S. Census Public Use Samples are used in Section IV to test these alternative hypotheses. The results suggest that successful military service does give a useful productivity signal to civilian employers. This signal is especially important for blacks and for those with less than high school educations. Thus, young men and women from disadvantaged backgrounds with above average productivity for their socioeconomic cohort may find the armed forces an effective means of identifying their talents to civilian employers.

The research described in this report also helps identify serious flaws in past attempts to measure the effect of military training on civilian wages. The analysis of wage changes over time emphasizes innate productivity differences between those who receive training in the military and those who do not, differences that may explain a good deal of the variation in wages between the two groups. No past study has adequately controlled for these differences because the data necessary for this task have only recently become available.

Meaningful estimates of the civilian returns to military training must be based on panel data in which individuals are followed through time. Some of the analysis in this report uses part of such a data set, the National Longitudinal Survey of Young Men; these data will soon be available in their complete ten-year form and will provide a unique resource for the study of military training and civilian wages. An analysis of young men's wages that makes full use of the time series component of these data promises to contrast sharply with previous findings on the returns to training received in the military. It may also point in substantiallly different policy directions than do previous findings.

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CONTENTS

I. INTRODUCTION

Young people influence their prospects for market earnings through decisions on when to leave high school, whether to attend college, whether to acquire vocational training, and so forth.[1] These decisions concern well-known examples of investments in human capital, but an often overlooked alternative is service in the armed forces. Military service deserves consideration as an investment alternative because when a number of other factors that influence wage rates (age and education, for example) are accounted for, veterans receive higher wages than do men who have never served in the military.[2]

If service in the military does improve post-service civilian wages, emphasizing that fact could be a useful military recruiting device. Young men or women who are uncertain about how much time they want to spend in the military will be more likely to join the armed forces on an experimental basis if they feel that their postservice wages will not suffer, and especially if military service actually improves future wages.

But, is the fact that veterans earn more than nonveterans proof that military service improves civilian wages? The analysis described in this report offers support for the following conclusion in this regard: Men who serve in the military receive higher civilian wages than nonveterans partly because they are innately more productive, but partly because they have accumulated human capital during their stay in the military. Whether differences in earnings due both to innate productivity and to human capital accumulation should be treated as returns to military service depends on whether workers actually have to serve in the military to capture these differences. For differentials due to military training the case seems clear. For innate ability, the answer depends on whether military service helps to sort out more productive workers from less productive workers. If service in the military supplies civilian employers with information about worker productivity that they might not otherwise have, such information may be valuable to enlistees, and would then represent a return to military service.

The research discussed in this report is concerned with the transferability to civilian occupation of training received in the military, and with the value of "certification" received through successful military service. However, a third factor may overshadow both training and certification effects as a determinant of the relationship between military service and civilian earnings. That factor is selectivity. The analysis presented in this report produces sometimes puzzling results unless one recognizes that veterans who receive training in the military are not a random cross-section either of veterans as a group or of the population at large. A full study of the selection process that results in young men becoming "trained veterans" is beyond the scope of this report, but the available evidence suggests a positive influence of military training on civilian wages when innate and unobserved productivity differences among young men have been accounted for.

The following sections attempt to put some of the costs and benefits of military service into perspective and to assess whether service in the military is a sensible investment for those not planning a career in the armed forces. The next section discusses alternative sources of wage differences between veterans and nonveterans and briefly reviews previous research on these differences. In Section III, some of these issues are explored using the 1966 through 1975 panels of the National Longitudinal Survey of Young Men. Section IV considers the roles of

[1]The list of supporting studies is now very long; see, especially, Becker, 1967; Mincer, 1962, 1974; Schultz, 1963.
[2]See Smith and Welch, 1974, for one example.

service in the military as a certification mechanism and as a source of human capital using the 1960 and 1970 U.S. Census Public Use Samples. Concluding comments are given in Section V.

II. BACKGROUND

For otherwise comparable individuals (same age, years of schooling, and so forth), differences in civilian earnings between veterans and nonveterans could arise from one or all of the following sources:

1. The selection process that occurs at induction into the military. This process acts through both self-selection—individuals choosing to enlist or to attempt to avoid service in the military—and the physical and mental screening process through which young men must pass before they can serve.
2. Formal training received in the military that improves civilian as well as military productivity.
3. The self-selection process that occurs at the end of the first term of military service whereby those with the best civilian (or worst military) alternatives tend to leave the military.
4. Formal training received under the auspices of the G.I. Bill.
5. For younger cohorts, a substitution of military for civilian job experience. In this case, the less substitutable military experience is for civilian experience, the less will be veterans' earnings relative to nonveterans' earnings.

If the physical and mental screening process were the source of observed earnings differences, the military could be acting in a certification or screening role for civilian employers. This certification effect, under certain conditions, may be considered a legitimate return to service in the military.

Another potential source of veteran/nonveteran earnings differentials, formal training, is the strongest candidate for treating service in the military as a source of human capital. If skills learned in the military are transferable to the civilian sector, military service is one means of "financing" human capital investment. This is especially true because the military is one of the few institutions in the United States that can levy a legal and binding claim on an individual's future earnings in return for the provision of formal training. This claim allows the military to do what other firms cannot do—to treat the future productivity of those in whom it invests as collateral for the invested resources.[1] A self-selection process that occurs at the end of the first term of service is also one potential source of the veteran premium (Massell, 1975). This effect is difficult to isolate empirically, and its importance, even its existence, has been questioned on logical grounds (see especially Lewis, 1974). As a consequence, the implications of point 3 are ignored in the remainder of this report, and it is mentioned here mainly for the sake of completeness.

Training received under the auspices of the G.I. Bill could also yield a return attributable to service in the military. Again, in this context the military would be acting as a source of funds for those who want to invest in additional training or schooling.

One of the major costs of acquiring a veteran premium may be the exchange of military on-the-job training or experience for civilian on-the-job training. If military and civilian on-the-job training are quite similar, then the cost is low. If service in the armed forces is not an especially good substitute for civilian work experience, then it is appropriate, indeed important, to recognize that time spent in the military is time withdrawn from civilian work

[1] If the military's wage policy is such that the present value of the opportunity cost of investing in human capital just equals the present value of the future benefits stream, then the return to the enlistee from human capital acquired in the services would be zero; the enlistee's post-service civilian wage would, however, be higher than in the absence of military service.

experience and civilian on-the-job training.[2] Partitioning the total earnings effect of veteran status into its constituent parts is no easy task. The best available micro-data are mostly cross-sectional, so direct "before and after" comparisons are seldom possible. However, in making cross-sectional comparisons between veteran and nonveteran earnings, one must always be conscious of the fact that men are not randomly assigned to each of these categories. Adequate statistical control for productivity-related personal characteristics becomes a key factor in this comparison because, as the subsequent analysis will show, many of these characteristics are correlated with veteran status.

Previous work in this area offers at best a confusing picture.[3] On one hand, results from several micro-data sources indicate that men of all ages who served in the armed forces earn more than men who never served, when individual characteristics such as schooling, age, and residence are held constant. On the other hand, several recent studies find that *training* received in the military has either a zero (Mason, 1970) or, in some cases, a negative (Jurkowitz, 1969) effect on civilian incomes.

These results apparently substantially reduce the set of competing explanations for the veteran premium suggested above. A zero rate of return to military training suggests that such training is specific to the armed forces and not transferable to civilian occupations. A negative training premium, however, hints at an alternative explanation: Previous studies may not have constructed adequate control groups against which to measure the performance of veterans with training. This view receives support from Norrblom's (1976) study of the return to military training for those who choose to use that training. Her study demonstrates clearly that for technical occupations, training received in the military is transferable to related civilian occupations.

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[2]See Cutright, 1972, on this point.

[3]See Norrblom, 1977, for an extensive review of the evidence on the influence of military service on civilian earnings.

III. VETERAN STATUS AND THE EARNINGS OF YOUNG MEN

OVERVIEW OF DATA

The National Longitudinal Survey (NLS) of Men Aged 14 to 24 in 1966 is a unique source of information on men of prime military service age. The initial survey design called for a five-year panel from 1966 to 1971, but the survey was later extended through 1976. At the writing of this report, the 1976 data had not been released, so the analysis presented below is based on the 1966 through 1975 panels only.

The scope of information gathered in the NLS varied year by year, and this variation affects the type of analysis that can be undertaken. In particular, 1971 was the last year of currently available data in which detailed military service information was gathered. Thus, for young men in the sample who entered the military after 1966 and were discharged after 1971 we know only that they are veterans; no information is available on the characteristics of their service—branch, training received, etc. For this reason, the working sample for the analysis is drawn from the 1971 panel, although results based on 1975 earnings information are also analyzed.

The narrow age range of those surveyed in this panel is both an advantage and a disadvantage in an analysis of veteran status and earnings. On the positive side, these data provide year-by-year labor force histories from the end of formal schooling to 1975 for many of those interviewed. This detailed work experience information makes the proxies for job experience used in many past earnings studies unnecessary and provides a clear picture of vocational and other training received both on and off the job.

The principal disadvantage of these data is that the panel covers only part of each individual's full age-earnings profile. If the consequences of veteran status for civilian earnings change as individuals age and accumulate work experience, the results described here may not tell a complete story. However, there is a considerable range of age and job experience in the panel (from ages 19 to 29 and 0 to 12 plus years of experience), and the analysis does consider the consequences of aging, job experience, etc. on the relationship between veteran status and earnings.

By way of general description, the 1971 sample contains responses from 3946 men. Table 1 illustrates the basic composition of the sample. One point immediately evident from this table is that stratification of the data (say, by branch of service) would lead to too few effective degrees of freedom for military-related comparisons. However, a shortage of degrees of freedom can in part be compensated for by large differences for comparisons of interest. Table 2 presents some of these differences for income and I.Q. categorized by age, then race. The rows and columns labeled "t(diff)" are t-statistics testing the statistical significance of differences between means of the various groups.

Table 2 indicates that gross income comparisons between veterans and nonveterans show little in the way of systematic differentials. Veterans do earn slightly more than nonveterans, but the difference is not significant at conventional levels. Further, only one of the 11 income comparisons for single years of age is statistically significant and it suggests that veterans actually earn less than nonveterans of similar age.

Among the several explanations that could account for this disagreement with past findings are that: (1) sample composition effects hide "true" veteran/nonveteran income differences; and (2) veteran "premiums" occur principally at ages not adequately represented in our sample. A complete analysis of compositional effects must await the multivariate analysis that

Table 1

SERVICE/NONSERVICE BREAKDOWNS[a]

	White		Black	
	Number	Percent	Number	Percent
Never in service	2141	72.4	805	81.4
Not in school[b]	1605		690	
Served in military	816	27.6	184	18.6
Not in school[b]	684		168	
Officers	32		1	
Trained in service	516	63.2	93	50.5
"Nonmilitary" trained[c]	368	45.1	57	31.0

[a]Indented categories are subgroups of the directly preceding nonindented category.

[b]At the time of the 1971 survey.

[c]Excludes those who received training considered useful only in the military.

follows, but the breakdowns of Table 2 appear to rule out age and race as possible explanations. The issue of when in a typical age-earnings profile veteran premiums are largest cannot be addressed directly with the NLS data (see, however, Sec. IV, below), but the year-by-year comparisons offer no suggestion of an increasing premium with age.

Table 3 is a glimpse of the selectivity of service in the military through a comparison of I.Q. levels between veterans and nonveterans; the picture that emerges is mixed. In the NLS data, "I.Q." is frequently more a measure of achievement than of innate ability (standard I.Q. test results and rescaled grade point averages are both included in the measure),[1] so some caution must be exercised in an interpretation of these comparisons. Taken at face value, however, the comparisons indicate that white veterans have slightly lower I.Q.s than white nonveterans, and black veterans and nonveterans have approximately equal I.Q. levels. To the extent that I.Q. as measured in the NLS data reflects human capital accumulation, these findings suggest that controlling for, say, educational attainment could produce more distinct veteran/nonveteran income differentials.

Veterans and nonveterans are grouped in Table 4 by their major activity the week before the 1971 NLS survey. Unemployment rates were a little higher for veterans than for nonveterans. However, veterans in this sample are about a year older than nonveterans, and because younger people are more prone to unemployment than older people, age-adjusting unemployment rates could lead to larger differences between veterans and nonveterans.

Finally, Table 5 gives a first look at a central issue in this study: the effect of training received in the military on post-service earnings. For this comparison, an individual is considered to have received training in the military if he was involved in any formal vocational training program other than officer candidate school, and programs classified as affecting productivity in the military only (skills not transferable to the civilian sector). In contrast to the served-nonserved earnings comparisons, the difference in age distribution between the two groups plays a dominant role in the overall income averages. Older veterans are more likely to have received training than men who served during the peak Vietnam War years (after 1966). When the overall average is "corrected" for age distribution differences,[2] trained

[1]See Kohen, 1973, App. E, for details.

[2]The correction consisted of weighting the "not trained" earnings figures by the "trained" age distribution and vice versa.

Table 2

Income Comparisons for Men Currently Not in School[a]

	Income by Age and Veteran Status											
		Age										
Status	19-29	19	20	21	22	23	24	25	26	27	28	29
Veteran	6290	3180	3430	4414	4837	5399	6436	7156	7970	7603	8719	8096
(s.d.)	(4004)	(1922)	(2366)	(2585)	(2829)	(2964)	(3316)	(3805)	(4813)	(3964)	(4789)	(4646)
(n)	(792)	(21)	(48)	(88)	(99)	(110)	(72)	(44)	(57)	(83)	(78)	(92)
Not veteran	6114	3638	4394	4940	4857	5962	6428	6929	6886	8444	8560	8426
(s.d.)	(4246)	(2591)	(2924)	(4205)	(3515)	(3458)	(3639)	(3765)	(4477)	(4445)	(4851)	(4999)
(n)	(2119)	(240)	(212)	(262)	(203)	(189)	(184)	(161)	(137)	(173)	(179)	(179)
t(diff)[b]	1.04	-0.79	-2.4	-1.4	-0.05	-1.5	0.02	0.4	1.5	-1.5	0.24	-0.53

	Income by Race and Veteran Status			
	White	Black	B/W Ratio	t(diff)
Veteran	6767	4315	0.64	8.9
(s.d)	(4110)	(2775)		
(n)	(638)	(154)		
Not veteran	6834	4359	0.64	14.8
(s.d.)	(4471)	(2987)		
(n)	(1502)	(617)		
Vet/not vet ratio	0.99	0.99		
t(diff)	-0.32	-0.18		

[a]Observations with zero income are included in this sample. Patterns do not change, however, when those with zero incomes are excluded.

[b]Test used differed depending on the outcome of a test of the equality of variances of the two means.

Table 3

I.Q. BY RACE AND VETERAN STATUS

Status	I.Q. White	I.Q. Black	B/W Ratio	Differential
Veteran	100.4	85.4		
(s.d)	(12.6)	(13.0)	0.85	-10.5
(n)	(526)	(97)		
Not veteran	103.0	83.5	0.81	-18.4
(s.d.)	(14.1)	(14.5)		
(n)	(1103)	(224)		
Vet/ not vet ratio	0.97	1.02		
t(diff)	-3.7	1.1		

Table 4

ACTIVITY LAST WEEK BY RACE AND SERVICE STATUS

Status	Served in Military Number	Served in Military Percent	Did Not Serve in Military Number	Did Not Serve in Military Percent
			White	
Currently working	668	81.9	1643	76.7
Unemployed	41	5.0	60	2.8
As a proportion of the labor force		5.8		3.5
Attending school	69	8.5	348	16.3
Other	38	4.7	90	4.2
			Black	
Currently working	148	80.4	565	75.2
Unemployed	19	10.3	57	7.1
As a proportion of the labor force		11.4		9.2
Attending school	7	3.8	81	10.1
Other	10	5.4	62	7.7

veterans earn more than veterans who received no training if the "trained" age distribution is used, but less if nontrained weights are used:

	Not Trained	Trained	Difference
Not-trained age distribution	$5651	$5752	-$101
Trained age distribution	$6902	$6636	$266

SOME BASIC CONSIDERATIONS

The analysis presented in this section is based on the Becker-Mincer human capital model of wage determination. The human capital earnings model postulates that each worker's wage

Table 5

VETERAN EARNINGS AND EDUCATION BY AGE AND TRAINING STATUS

	Training Status[a]					
	Not trained			Trained		
Age	Earnings	Education	(n)	Earnings	Education	(n)
19	3084	11.8	(15)	2877	11.1	(15)
20	3560	11.7	(29)	3131	11.7	(24)
21	4483	11.8	(59)	4230	11.6	(39)
22	4581	12.3	(69)	4643	12.1	(56)
23	5127	12.4	(68)	5040	12.3	(60)
24	6279	13.2	(46)	5962	12.9	(40)
25	6448	14.1	(25)	7050	13.5	(31)
26	7517	13.6	(27)	7938	13.2	(36)
27	7173	12.9	(25)	8025	12.4	(63)
28	8550	13.4	(24)	9465	12.6	(56)
29	8482	12.7	(25)	9222	12.8	(63)
All	5651	12.6	(412)	6636	12.42	(443)

[a]Trained veterans are those who received any type of formal training while in the military except "military only"; training classified as "military only" is assumed to be of value only in the service.

is in part determined by the "price" employers are willing to pay for personal characteristics that enhance productivity, and the quantity of those characteristics "held" by the worker. For example, if formal schooling improves productivity, employers will bid (in a competitive market) for employees with higher levels of education by paying a wage premium for each additional year of schooling; in the same vein, if on-the-job training (OJT) increases productivity, then workers with more job experience and thus more OJT should receive higher wages than workers with less job experience.

One means of formalizing this model is through the following equation:

$$\text{Ln}(w) = a_0 + a_1 E + a_2 X + \sum_i a_i OF_i$$

This equation relates the natural log of an individual's wage rate (Ln(w)) to his or her education level (E), job experience (X), and levels of other factors (OF_i) that may affect wages and productivity (veteran status or training in the military, for example). With this formulation the coefficients (the a_i) are interpreted as percentage changes in wages received as a result of one unit change in the explanatory variable.[3]

Early studies of the relationship between market wages and human capital were often plagued by severely limited data.[4] Key among these limitations was an incomplete measure of human capital accumulation through formal and on-the-job training. The NLS sample offers improvement in both of these areas because it contains a detailed record of vocational training histories and records on a year-to-year basis time allocation among school, work, and unemployment. Because men in this sample are young, the time allocation information in the

[3]Under special circumstances (see Mincer, 1974), the coefficients on these variables can be interpreted as "rates of return" to acquiring an additional year of schooling or an additional year of on-the-job training, but this interpretation is not essential to this analysis.

[4]The seminal works on earnings functions are those of Mincer, 1962, 1970, and 1974; Becker, 1967; and Hanoch, 1967.

survey covers a substantial portion of total "work history." This represents an important gain over studies limited to OJT measures based on total time since leaving school.

Initially, two working samples were selected from the 1971 panel of the NLS file. The first consisted of all whites and the second of all blacks who were not enrolled in school in 1971 and who worked during 1971. The white sample contains 1275 observations, 241 of whom are veterans, and the black sample contains 427 observations with 40 veterans.[5] Because of the limited number of black veterans, the analysis in this section concentrates on the white sample; however, a complete set of regression results for blacks are given in Appendix B.

Table 6a lists and defines the principal variables used in this analysis. The selection of variables was based on an extended version of the standard human capital earnings model; variable construction is straightforward, with the possible exception of the measure of total work experience. Work histories before the first year of the survey (1966) are sometimes incomplete, especially for those who changed jobs frequently. As a consequence, a fairly lengthy procedure, described below, was developed to ensure maximum accuracy for pre-1966 job experience; tests were also performed for the sensitivity of results to alternative experience measures.

Table 6b presents means and standard deviations for key variables. In 1971, the average worker in the white sample earned $3.91 an hour, was a little over 24 years old, and had accumulated more than five years of labor force experience. About half the sample had received some form of vocational training, one-fifth were veterans, and one-tenth reported some form of disability that limited either the type or amount of work they could perform.

As a point of comparison, Table 7 presents the results of two alternative human capital earnings functions. These equations are distinguished from other wage regressions principally by work experience measures based on actual time worked in the past. The measure of labor force experience used in these regressions consists of actual labor force experience after 1966 plus an estimate of pre-1966 experience. Pre-1966 experience is estimated from information on years with current employer and first job after leaving school. When no information was available, an individual was treated as if he worked every year since six months after his high school graduation date. Each of these years was then weighted by weeks worked in 1966 to arrive at a measure of pre-1966 labor force experience.

These equations are consistent with predictions from theory and with empirical results from previous human capital studies: More schooling leads to higher wages, although the implied "rate of return" to investments in schooling (4.9 percent) is lower than figures reported in earlier studies; those suffering from a work-related disability command wages that are 9 percent lower than other workers; southern wages are 13 percent less than wages in other parts of the country; and workers in SMSAs earn 15 percent more than non-SMSA workers.

Because specific work experience information is usually not available in micro-data sets, earnings function estimates often combined the effects of age and experience on earnings. The NLS data provide a basis for separate estimation not only of age and job experience effects, but of the effects of different types of experiences on wages. Equation (2) suggests that at least three time-related characteristics of workers may affect wages—total time spent working, the number of years worked for current employer, and age. Both experience variables exhibit the traditional curvilinear relationship with wages, although the restricted age range of the sample cautions against placing too much weight on the estimated parameters. Taken together, these estimates suggest that a young worker of average age, work, and employer experience for this sample will earn about 8 percent more through the combined effects of those factors if he works an additional year for his current employer.

Again confirming previous findings, the regressions in Table 7 indicate a near 10 percent wage premium for those in the sample who are veterans, controlling for the other work-related and personal characteristics.

[5]See Appendix A for details of the sample selection procedure.

Table 6a

Definition of Variables

Variables	Definition
Dependent Variables	
WAGE	Hourly wage rate; calculated using "usual" hours per week, weeks per year, and earnings when not reported in dollars per hour.
LNWAGE	Natural log of wage.
Explanatory Variables	
EDUCATION	Highest year of formal schooling completed.
AGE	Age in 1971 or 1975.
EXPERIENCE	Years of actual work experience since 1965 plus estimated years before 1966.
EMPLOYER EXPERIENCE	Years of actual work with current employer.
VOCTRAIN	A binary variable that equals 1 if any vocational training, 0 otherwise.
VETERAN	A binary variable that equals 1 if individual served any time in the armed forces, 0 otherwise.
DISABLED	A binary variable that equals 1 if individual claims a disability that affects work, 0 otherwise.
REGION	A binary variable that equals 1 if region of residence is South, 0 otherwise.

Table 6b

Means and Standard Deviations

(Whites, 1979, n = 1275)

Variable	Mean	Standard Deviation
WAGE ($)	3.91	1.68
EDUCATION (years)	12.60	2.61
AGE (years)	24.45	3.15
EXPERIENCE (years)	5.85	2.75
EMPLOYER EXPERIENCE (years)	2.83	2.64
BINARY VARIABLES (proportions)		
Vocational training	0.50	
Veteran	0.19	
Disabled	0.10	
Region (South = 1)	0.29	

Table 7

BASIC WAGE EQUATIONS
(Dependent variable: ln(Wage), n = 1293;
t-ratios in parentheses)

Explanatory Variable	Coefficient	
	Eq.1	Eq. 2
Education	0.049	0.047
	(10.2)	(10.0)
Experience	0.10	0.069
	(5.6)	(3.6)
$(\text{Experience})^2$	-0.0052	-0.0033
	(-4.2)	(-2.6)
Years with current employer		0.064
		(5.7)
$(\text{Years with current employer})^2$		-0.0052
		(-4.4)
Age	0.017	0.016
	(2.9)	(2.8)
Disability	-0.090	-0.090
	(-2.7)	(-2.7)
Vocational training	0.025	0.022
	(1.2)	(1.1)
Veteran	0.095	0.095
	(3.4)	(3.4)
Region (1 = South)	-0.13	-0.12
	(-5.5)	(-5.2)
SMSA	0.15	0.15
	(7.0)	(7.1)
Intercept	-0.21	-0.16
R^2	0.29	0.31
F	57.7	52.0

CIVILIAN EARNINGS AND VETERAN STATUS

To this point, civilian-military comparisons have been summarized in a single variable that distinguishes between veterans and those who never served in the armed forces. Because the NLS data contain information on military experience beyond veteran status, it is possible to decompose the effect of veteran status into several parts.

Military-related information available in the NLS is summarized in Table 8 for the sample of 1275 white working males. A quarter of this sample (327 observations) attempted to enlist in the services or were drafted and failed to meet either or both of the physical and mental standards required for entry into the military; of these, the vast majority were rejected on physical grounds. A fifth of the sample (241 observations) claimed veteran status; of the veterans, 20 percent were drafted and 61 percent served in the Army. Average length of service for veterans in this sample was 20.1 months with half the veterans serving 20 months or more.

Over 40 percent of those who served in the armed forces (109 observations) received some type of professional or vocational training. Because we are interested in the effect of training in the military on civilian earnings, veterans who were trained in areas classified as useful only in the military are treated as though they received no training. For those familiar with such figures, the 45 percent figure may seem low, but the age restriction on the NLS data yields a sample of veterans in which short terms of service are overrepresented. In fact, 35

Table 8

SUMMARY STATISTICS FOR MILITARY VARIABLES

Category	Proportion or Mean	Base
Rejected from military	0.30	1293 Sample[a]
On physical grounds	0.24	"
On mental grounds	0.01	"
On physical and mental grounds	0.004	"
Other reasons	0.04	"
Veteran	0.19	"
Drafted	0.20	All veterans
Served in Army	0.61	"
Length of service	20.1 months	"
Served more than 20 months	0.50	"
Received training in service[b]	0.45	"
Length of training	4.4 months	Vets with training
Type of training		
Professional/technical	0.30	"
Managerial	0.03	"
Clerical/sales	0.11	"
Skilled manual	0.38	"
Other	0.18	"

[a]White, working males not in school in 1971.

[b]Excludes those who received training classified as "military only."

percent of those claiming veteran status served six months or less; if all trainees are assumed to have served seven or more months, then fully 70 percent of veterans with more than six months of service received some form of training.

The last part of Table 8 categorizes military training by type—professional, managerial, clerical, skilled manual, and other. Cell sizes become disturbingly small when trainees are subclassified by type of training; as a consequence, some care must be taken in interpreting regressions in which categories for type of training enter as explanatory variables.

Table 9 presents the consequences for civilian earnings of various aspects of military service (or nonservice). This table contains the partial results of seven alternative regression specifications. These specifications take Eq. (2) of Table 7 as a starting point and add information on those who served in or were rejected from the armed forces. For ease of presentation, Table 9 gives only those coefficients related to military variables. For the most part the addition or deletion of military variables only marginally affects either the size or significance of other variables in the equation.

The basis for comparisons is Eq. (1), in which the veteran status coefficient indicates a veteran premium of 9.5 percent. But, as indicated above, veterans in this sample fall into two groups: those who served six months or less and those who served more than six months. This distinction is important primarily because veterans with six months or less of service are likely to be reservists; their experience with the military will differ considerably from veterans who were full-time members of the armed forces.

In Eq. (2) of Table 9, veterans are grouped into two classes depending on their length of service. The results suggest that veterans with very short terms of service can command a higher "premium" than veterans who were properly in the military, but the two coefficients are not different at conventional significance levels ($t = 1.09$). However, this distinction between reservists and others is useful in subsequent specifications, and is maintained for that reason.

Table 9

Effect of Military Variables on Civilian Earnings

(t-ratios in parentheses)

Explanatory Variable[a]	Coefficient 1	2	3	4	5	6	7
Veteran[b]	0.095						
	(3.4)						
Served six months or less[b]		0.13	0.13	0.13	0.12	0.12	0.12
		(3.2)	(3.2)	(3.2)	(3.1)	(3.1)	(3.1)
Served seven months or more[b]		0.069	-0.063	0.059	0.049	0.11	0.11
		(2.0)	(-0.4)	(1.5)	(1.4)	(2.0)	(2.0)
Served seven to 24 months[c]			0.0073				
			(1.0)				
Served 25 to 36 months[c]			-0.0060				
			(-0.8)				
Served 37 months or more[c]			0.0012				
			(0.2)				
Drafted				0.034			
				(0.6)			
Trained in military						-0.089	-0.092
						(-1.4)	(-1.3)
Months of military training							0.0007
							(0.1)
Rejected from military:							
for physical reasons					-0.015	-0.015	-0.015
					(-0.6)	(-0.6)	(-0.6)
for mental reasons					-0.32	-0.32	-0.32
					(-3.2)	(-3.2)	(-3.2)
for physical and mental reasons					-0.54	-0.54	-0.54
					(-3.3)	(-3.3)	(-3.3)
for other reasons					-0.020	-0.01	-0.01
					(-0.2)	(-0.2)	(-0.2)

[a]For other variables included in regression, see Table 7 7, Eq. 2.

[b]Binary variable.

[c]Continuous variable.

If one were to predict the consequences for young men's wages of a tour of duty in the military, the loss of civilian job experience would be a prime candidate to influence that prediction. The effect on wages of the forgone experience depends on how well time spent in the service substitutes for civilian job experience. Equation (3) estimates the effect of length of service on civilian wages using a piecewise linear functional form. Length of service is categorized into three segments: 7 to 24 months, 25 to 36 months, and 37 months and over. Within each of these segments, an additional month of service has the same percentage effect on wages (for example, an increase in service length from 8 to 9 months will raise wages by the same percentage as an increase from 19 to 20 months), but this percentage varies among segments.

In Eq. (3), time spent in the military neither improves nor worsens civilian wages. None of the individual slope coefficients for the three length of service variables is significantly different from zero, and a test of the hypothesis that the inclusion of those variables significantly increases the equation's explanatory power fails at conventional levels ($F = 0.43$).[6]

The majority of veterans in the NLS served during a ten-year period beginning in about 1960. This period contains the Vietnam buildup, and as a consequence, the fraction of veterans who were drafted changed dramatically during this period. Draftees may differ from enlistees in innate and acquired characteristics that may not be recorded in these data, and Eq. (4) tests the hypothesis that the way a veteran entered the armed forces affects his subsequent civilian wage. The coefficient on the variable identifying those who were drafted is not different from zero at conventional statistical levels, so enlistees and draftees fare about equally well in terms of market wages after they leave the service.

Among the possible sources for a veteran premium is the presence in the nonveteran population of those who failed to pass either the mental or the physical armed forces entrance exam. In a sense, these people have been identified as being of below-average productivity (or potentially so), and their inclusion in the nonveteran group will draw down average productivity for that group. Equation (5) indicates that rejectees are not a major source of observed differences between veterans and nonveterans. Among rejectees, the only significant group in terms of numbers is the group who failed to meet the physical standards for entry into the armed forces. There were 309 such individuals in the 1275 sample, and when other characteristics are controlled for, Eq. (5) indicates no difference between those rejected for physical reasons and other nonveterans. This may be explained by the fact that the regression also contains a variable identifying young men who claim a work-related disability. However, only 62 observations claimed both a work-related disability and a rejection on physical grounds, so the majority of those rejected from the military on physical grounds did not suffer losses in civilian earnings compared with other nonveterans.

Although wages of civilians and rejectees do not differ, the inclusion of the rejection variables in Eq. (5) does affect the size and significance of the coefficient for veterans who served more than six months. When the comparison group is all nonveterans, veterans with more than six months of service average about 7 percent more in hourly wages; but when rejectees are removed from the comparison group, as is the case in Eq. (5), the wage premium to long-term veterans drops to 4.9 percent and is not significantly different from zero at conventional levels. This movement is inconsistent with the fact that the majority of those rejected from the armed forces earn no less than those who were not rejected (and who never tried to enter); but in and of itself, it points to the possibility that the screening through which veterans must pass at induction may be one cause of the estimated premium to veteran status in data sets lacking information on those rejected from military service.

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[6]Note that in Eq. (3), the effect of veteran status becomes $d\ln(w)/d(\text{SERVED } 7+) = -0.063 + 0.0073(\text{SERVED } 7\text{-}24) - 0.0060(\text{SERVED } 25\text{-}36) + 0.0012(\text{SERVED } 37+)$; for a length of service of 24 months this expression yields a coefficient of 0.11, which is significantly different from zero at conventional levels.

The last two equations in Table 9 consider the effect on post-service wages of training received in the armed forces. In Eq. (6), a binary variable with the value 1 for those who received training in the military, 0 otherwise, is included in the regression; and in Eq. (7), the binary variable and a variable measuring number of months of training enter the regression. Regardless of the form of the equation, the results are puzzling. Although of weak statistical significance, training received in the military seems to lower civilian wages. Even those in the sample who said that they used military training on their civilian job did not command significantly higher wages than other similar individuals.[7] Furthermore, separating out trained veterans yields a significant positive veteran premium for untrained veterans, a result that flies in the face of intuition and economic theory.

There are many potential explanations for the counterintuitive behavior of the military training variables. A likely candidate, and unfortunately one of the most difficult to test for, is the possibility of selectivity bias. Those who received training in the military may differ from those who did not in innate and unobserved ways that lower their market productivity; in other words, neither veterans without training nor nonveterans are adequate control groups against which to assess the wage rates of trained veterans. This view receives some support from the almost significant *negative* coefficient on the training dummy variable; one could conceive of a zero coefficient, but the prospect that training received in the military actually reduces productivity in the civilian sector is difficult to accept.

A second class of explanations for the negative, or insignificant, relationship between military training and civilian wages stems from the restricted age range for the NLS sample. It may be true that recipients of training in the military initially receive below average civilian wages while they combine this training with heavy doses of on-the-job training; but, because of military and on-the-job training investments, wages of trained veterans may rise more quickly than the wages of others in this sample. If one could obtain observations on individuals in the NLS sample at later points in their life cycles, there might be a positive differential between trained veterans and others, even though the relationship between wages and military training is either zero or negative at younger ages.

One implication of this explanation is that the effect of service in the military, especially of training received, ought to vary with the amount of time an individual has spent in the civilian labor force since military service. If young veterans require an adjustment or investment period upon returning to civilian life, the consequences of veteran status for civilian earnings ought to depend positively on the length of time since the service. Equations (1) and (2) in Table 10 test this hypothesis both for all veterans and for veterans who received training in the military. The results reject this line of reasoning as an explanation for the negative coefficient on military training: An increase in the number of years since military service is associated with marginally significant declines in observed wages.

Other tests were performed in search of an explanation for the weak showing of the military training variable. These included the estimation of a variety of different functional forms that allowed months of training and years since military service to affect wages nonlinearly, the inclusion of interaction terms between military variables and age and education levels, and a reclassification of the military training variable into five subcategories defined by the type of training received in the military (professional, managerial, clerical, manual, and other). None of these attempts shed new light on the question at hand, and they are not reported. With the exception of the two binary variables identifying veterans who served six months or less and more than six months, no combination of variables describing the char-

[7]The addition of a variable identifying those who claim to use military training in their civilian job produced a positive but insignificant coefficient ($b = 0.03$; $t = 0.44$); however, the total effect on civilian wages of training received in the military, for those who used that training, is the sum of the "used training" coefficient *and* the "received training" coefficient, which is negative, although insignificant.

Table 10

YEARS SINCE SERVED RESULTS

Explanatory Variable[a]	Coefficient	
	Eq. 1	Eq. 2
Served six months or less[b]	0.13	0.13
	(3.1)	(3.2)
Served seven months or more[b]	0.035	0.086
	(0.2)	(0.5)
Served seven to 24 months[c]	0.011	0.0078
	(1.3)	(1.0)
Served 25 to 36 months[c]	-0.0017	-0.0027
	(-0.2)	(-0.3)
Served 37 months or more[c]	-0.0010	-0.0004
	(-0.1)	(-0.1)
Drafted	-0.055	-0.033
	(-0.4)	(-0.7)
Trained in military	-0.093	-0.16
	(-1.2)	(-1.2)
Months of military training	0.0023	0.00055
	(0.03)	(0.1)
Years since served in military	-0.021	-0.025
	(-1.4)	(-1.2)
(Years since served) x (trained)[d]		0.017
		(0.7)

[a]For other variables included in regression, see Table 7, Eq. 2.

[b]Binary variable.

[c]Continuous variable.

[d]Interaction of "Years since served in military" and "Trained in military."

acteristics of military service—training received, amount of training, years since service, length of time served, etc.—significantly improves the explanatory power of the wage regression.

1975 WAGE ANALYSIS

The currently available NLS data for young men contain information on wages and work histories through 1975. The principal drawback of the 1975 panel is its lack of detailed information on the nature of military service for veterans, hence to this point the focus of analysis has been on the 1971 results. But the existence of 1975 wages for a subset of the 1971 sample permits a statistical experiment that may provide evidence on the selectivity argument put forth in the preceding section.

Selectivity problems arise when unobserved and sometimes unobservable characteristics affect wages and are correlated with explanatory variables used in an analysis. In this case, the prospect that training in the military might lead to lower civilian wages is sufficiently unlikely to suggest an alternative explanation: It is not military training itself, but something about those who received it, that causes the negative and nearly significant relationship between post-service wages and training. One candidate among these unobserved characteristics is the innate productivity of those who receive military training.

The time-series component of the NLS data provides the ingredients for a crude but informative test of this proposition. Put most simply, each individual who worked in both 1971 and 1975 can act as his own "control" group; that is, changes in wages from 1971 to 1975 can be conditioned on levels of wages in 1971. Individuals who are innately less productive than average will have lower than average 1971 wages, but the rate of change in their wages over time ought still to depend in part on previous and ongoing human capital investments. In particular, training received in the military ought to promote wage growth between 1971 and 1975.

Table 11 presents the results of this test using Eq. (2) of Table 10 as a point of reference. Assessing the effect of military training on wage growth between 1971 and 1975 involves the evaluation of a multi-termed partial derivative, but it is instructive first to review the behavior of other variables in the regression. Those variables with human capital interpretations perform as that theory would predict. For example, formal schooling is positively associated with the rate of wage change between 1971 and 1975, which is consistent with the frequently held view that schooling and on-the-job training are complements in the human capital investment process. Age, in contrast, is negatively associated with wage change during this period, which is again consistent with the predictions of the basic framework: Older workers in this sample will invest less in acquiring new human capital, and their rate of wage change over time will be low relative to younger, more investment inclined workers.

In the regressions based on 1971 wages, vocational training had a small and insignificant effect on wage levels (see Table 7). However, the receipt of vocational training before 1971 significantly affects wage growth between 1971 and 1975. These contrasting results are particularly interesting in light of the selectivity arguments that motivate this section, and more will be said of them in the discussion of the military training results.

The form of this regression allows military training to affect wages in several ways: through a once-and-for-all effect that depends only on whether a person received military training, through months of military training received, and through an interaction between whether a veteran received training and the length of time since he served in the armed forces. The effect of having received training in the military on the rate of wage change is thus:

$$\frac{\partial \ln(w_{75})}{\partial MT} = -0.26 - 0.0039(MONS) + 0.041(YSS),$$

where MT is the binary variable indicating whether a person was trained in the military, MONS is months of training, and YSS is years since that person served in the military. The following table evaluates this expression for various levels of MONS and YSS:

MONS	YSS	Coefficient	s.e.	t-Ratio
4.3[a]	6	-0.032	0.11	-0.29
4.3[a]	8.8[a]	0.082	0.08	1.03
4.3[a]	10	0.13	0.09	1.44
1	10	0.15	0.10	1.50

[a]Mean values.

Table 11

1975 WAGE REGRESSION

Explanatory Variable	Coefficient	(t-ratio)
Education	0.023	(4.0)
Experience	0.021	(0.7)
(Experience)^2	-0.0007	(-0.6)
Years with current employer	0.01	(0.93)
(Years with current employer)^2	-0.00004	(-0.1)
Age	-0.014	(-2.1)
Disability	-0.062	(-1.2)
Vocational training	0.054	(2.3)
Veteran < 6 months	-0.043	(-1.0)
Veteran < 7+ months[a]	0.082	(0.3)
Served 7-24 months[b]	-0.0006	(-0.1)
Served 25-36 months[b]	-0.0074	(-0.7)
Served 37+ months[b]	-0.0064	(-0.7)
Drafted	-0.038	(-0.4)
Trained in military	-0.26	(-0.9)
Months of military training	-0.0039	(-0.4)
Years since served	-0.0052	(-0.2)
(Years since served) x (trained)[c]	0.041	(1.3)
ln(1971) wage)	0.65	(21.7)
Region (1 = South)	-0.03	(-1.2)
Intercept	0.76	(4.6)
R^2	0.47	
F	37.3	
N	87.2	

[a]Binary variable.

[b]Continuous variable.

[c]Interaction between years since served in the military and trained in the military.

These results contrast sharply with estimates based on cross-sectional data given in Table 10. For the 1971 cross-sectional regressions, evaluation of the effect of military training at mean values for months trained and years since service yields a coefficient of -0.073 (t-ratio $= -1.08$). Because t-ratios never reach conventional levels of significance, these comparisons only suggest the direction future work should take. A more complete analysis of the NLS data that takes full advantage of the time-series component of those data seems likely to yield important new insights into the link between military training and post-service earnings. Such a study should be of the first priority when the 1976 NLS panel becomes available.

A comparison of the coefficients for vocational training in Tables 10 and 11 supports the selectivity argument. The small and insignificant coefficient for vocational training in Table 10 could result from a selection process in which those with below-average innate productivity seek out and invest in vocational training. However, when one controls for basic differences in productivity among individuals, as the 1975 regression does, although crudely, vocational training has a significant and positive effect on wage growth. Thus, both the military and vocational training results support the role of selectivity as an important determinant in cross-sectional data of estimated relationships between certain types of human capital investment and wage rates.

III. A TEST OF VETERAN STATUS AS A SCREENING DEVICE

Service in the military provides civilian employees with information on veterans in two ways: First, a person must pass through a series of mental and physical examinations to qualify for the military; and second, a veteran must meet certain minimum standards of behavior and performance to receive an honorable discharge from the military. In this section, data from the 1960 and 1970 Census Public Use Samples are analyzed in an attempt to associate an overall "screening" effect with service in the military.

The question is not only whether individuals who enter the military are, on average, more productive than individuals who cannot or do not enter the military, but rather whether this information is of value to civilian employers in choosing among potential employees.[1] The difficulties in testing this proposition are twofold: First, there is the issue of the value of the screen—does it supply independent information, or would those who successfully pass through the screen have earned more in any case. And, second, more able people may find it profitable to pursue human capital investment strategies that differ from strategies followed by less able people. If individuals who successfully pass through a given screening mechanism choose to invest heavily in certain forms of human capital, then a positive relationship between their wage rates and the fact that they passed through the screen may be due mainly or entirely to human capital investment activities.[2] As discussed below, this correlation between ability and human capital can serve as the foundation for a test to distinguish between the human capital and screening explanations for the veteran premium.

The micro-data set on which the test for a screening effect of service in the armed forces is based identifies only individuals who served in the military; in the nonveteran population men rejected from the military cannot be distinguished from men who were never called up or never tried to enlist. Assume for the moment that the proportion of people who claim veteran status—that is, the proportion of people in a given age group subjected to screening by the military who successfully passed through the screen—is a linear function of the proportion of people called up.[3] It then follows that if veteran status does supply information on employee ability to civilian employers, the wage differential between veterans and nonveterans should be a positive function of the proportion of men claiming veteran status.

The explanation for this correlation stems from what employers can sensibly assume about the makeup of the *non*veteran population when those who could become veterans are inherently more productive than those who could not. An employer who knows nothing about two prospective employees except that one is a veteran and one is not should choose between, or assign wages to, these men on the basis of the *expected* veteran status of the nonveteran. But the nonveteran population consists of three groups: men who were actually rejected, men who could become veterans if they chose to (potential acceptees), and men who could not become veterans because of their inability to meet the required mental and physical standards (potential rejectees). If a high proportion of a particular subgroup consists of veterans, and rejection rates are nonnegligible, employers will not be far off the mark if they assume that all nonveterans are rejectees. In contrast, if only a small proportion of men in the subgroup are veterans,

[1]Service in the military, for example, might provide civilian employers with productivity information that would require some substantial passage of time to acquire in the absence of military service. How important this information is depends on how much civilian employers would have had to spend to produce the same information.

[2]The issue in a nutshell is whether an employee or potential employee would choose to expend resources to pass through a particular screen successfully if he knew that the screening mechanism itself produced no net human capital.

[3]This assumption requires that there are no major fluctuations in the standards used to select men for service in the military.

it would be inappropriate for employers to assume that all nonveterans are rejectees. Therefore, in populations in which only a small portion of men are veterans, the screening value of veteran status is low because the nonveteran population will contain a substantial number of men who could qualify as veterans if they chose to do so. In this low veteran status population, the partial (other things equal) differences between veteran and nonveteran wage rates (the veteran premium) will be small relative to this same difference in a high veteran status population. To summarize, if veteran status helps employers distinguish between high- and low-productivity individuals, then:

- Other things equal, the effect of veteran status on civilian earnings will be a positive function of the proportion of men in a given population who claim veteran status.

This is not a hypothesis that distinguishes between screening and human capital explanations of the veteran premium. Productivity could be directly and easily measured without screens such as veteran status, and yet we might still observe a positive relationship between the effect of veteran status on earnings and the proportion of men who are veterans. This would be true so long as, for whatever reason, veteran status were positively correlated either with an unmeasured element of an individual's human capital or with his ability. In its simplest form, a test of this hypothesis establishes only whether, on average, veteran status categorizes individuals into more and less productive groups holding constant such observable characteristics as age, schooling, and residential location. Confirmation of this hypothesis is a necessary precondition for the more discriminating tests of screening versus human capital explanations that follow.

The data used to test this initial hypothesis were drawn from the 1960 and 1970 1-in-100 Census Public Use Samples and consist of all black and a comparable number of white civilian men between the ages of 22 and 65 for whom an hourly wage can be calculated.[4] These data were stratified into 11 four-year age groups[5] and the following equation calculated for each group:

$$\ell nw = a_0 + a_1 Ed + a_2 AGE + a_3 VET + a_4 METRO + a_5 CENCITY + a_6 SOUTHRES + u,$$

where Ed = year of schooling,
AGE = Age in Census Year,
VET = 1 if veteran,
METRO = 1 if residence in a metropolitan area,
CENCITY = 1 if residence in a central city,
SOUTHRES = 1 if residence in South,
u = error term.

The results of these equations for the veteran status coefficients are given in Table 12.[6] The overall picture is one of a positive premium to veteran status with a value as high as 10 percent for whites and 9.4 percent for blacks. There are a few cases of negative returns to veteran status, but their concentration in young age groups suggests that they result from a negative correlation between veteran status and civilian labor force experience that seems likely to hold for men under 30 years of age. The table further illustrates the substantial variation among age groups in the proportion of men claiming veteran status—a necessary,

[4]The self-employed were one of the major groups excluded.
[5]For veteran status to have informational value within these age groups, we need only assume that the elasticity of substitution among workers is higher within than between age groups.
[6]Results based on two-year age groups are given in Appendix C.

Table 12

EFFECT OF VETERAN STATUS ON HOURLY WAGE BY AGE: FOUR YEAR AGE GROUPS

Race/Age Group	Veteran Status Coefficient	t-ratio	Percent Veteran	N
Whites, 1970				
22-25	-0.044	-2.18	0.43	3994
26-29	-0.042	-2.65	0.46	4280
30-33	0.023	1.35	0.54	3574
34-37	0.056	2.98	0.67	3296
38-41	0.035	1.75	0.71	3362
42-45	0.084	3.84	0.80	3555
46-49	0.024	0.94	0.83	3511
50-53	0.040	1.91	0.72	3135
54-57	0.020	0.96	0.44	2755
58-61	0.0094	0.34	0.30	2335
62-65	0.019	0.43	0.21	1443
Whites, 1960				
22-25	-0.062	-2.71	0.44	2756
26-29	0.055	2.82	0.69	3246
30-33	0.081	4.32	0.74	3597
34-37	0.067	3.22	0.81	3563
38-41	0.098	4.66	0.77	3454
42-45	-0.0081	-0.45	0.55	3235
46-49	0.0071	0.35	0.35	2976
50-53	-0.016	-0.66	0.29	2519
54-57	-0.042	-1.28	0.16	2248
58-61	0.059	1.70	0.21	1796
62-65	0.15	4.13	0.46	1233
Blacks, 1970				
22-25	0.017	0.62	0.31	3443
26-29	0.028	1.21	0.27	3389
30-33	0.068	2.90	0.33	3041
34-37	0.035	1.56	0.50	3116
38-41	0.038	1.62	0.56	2971
42-45	0.053	2.12	0.63	3010
46-49	0.090	3.56	0.69	2852
50-53	0.062	2.29	0.55	2472
54-57	0.026	0.94	0.36	2097
58-61	0.062	1.75	0.23	1773
62-65	0.072	1.33	0.18	1115
Blacks, 1960				
22-25	-0.035	-1.10	0.27	2440
26-29	-0.28	-1.11	0.44	2721
30-33	0.068	2.87	0.53	2906
34-37	0.085	3.76	0.61	3091
38-41	0.047	2.07	0.57	2914
42-45	0.031	1.19	0.40	2533
46-49	0.024	0.82	0.29	2454
50-53	-0.017	-0.47	0.23	2069
54-57	-0.069	-1.44	0.13	1725
58-61	-0.017	-0.30	0.12	1338
62-65	-0.0057	-0.10	0.35	798

but as we shall see not sufficient, condition for a discriminating test of the veteran screening hypothesis.

To test the hypothesis given above, veteran status coefficients from each age group were regressed on the proportion of men claiming veteran status in each group.[7] The results of these regressions are given in Table 13.[8] All equations indicate a positive relationship between the proportion veteran and the effect of veteran status on civilian wage rates, although the F-statistic for one equation—blacks, 1970—is not significant at conventional levels.[9]

Table 13

VETERAN COEFFICIENT REGRESSIONS: FOUR-YEAR AGE GROUPS[a]
(t-ratios in parentheses)

	Explanatory Variables				
Race/Age Category	Proportion Veteran	Intercept	R^2	F	N
Whites, 1970 22-65	.165 (2.72)	-.078	.45	7.40	11
Whites, 1960 22-65	.171 (2.46)	-.062	.40	6.04	11
Blacks, 1970 22-65	.059 (1.38)	.021	.17	1.90	11
Blacks, 1960 22-65	.244 (4.05)	-.081	.65	16.40	11

[a]Variables weighted by 1/(s.e. of veteran coefficient).

An evaluation of veteran status as a screening device depends on the degree to which the results in Table 13 could be explained by alternative hypotheses. Can a positive relationship between proportion veteran and the effect of veteran status on earnings be explained by human capital investment behavior? In Fig. 1, the proportion of men claiming veteran status in each group is plotted against age; purely by coincidence, the resulting profiles look remarkably like a typical life-cycle earnings profile. This is especially true for the 1970 data where the highest value for proportion veteran occurs for men in their mid 40s, which also tends to be the peak earnings age. The relationship between proportion veteran and earnings may therefore be a spurious one based on the inverted U shape of veteran status and earnings across age groups. This would be especially true if more able men invested more heavily in human capital at early stages of their life cycles than less able men. Then, only because of differential human capital investment paths, veterans (i.e., more able men) will have fairly low

[7]Regressions are weighted by the inverse of the standard error of the dependent variable ($\partial \ell n\ w/\partial Vet$) when weighting significantly improves the efficiency of the equation (that is, weighting results in a significant increase in the F-statistic associated with the equation). See Saxonhouse, 1976, on this procedure.

[8]Other functional forms (quadratic, semi-log, etc.) were also explored but the linear specification provided the best overall fit.

[9]In fact, the results for blacks are somewhat puzzling overall. Although I have not made a systematic study of the screening process itself, it may be that standards were more variable over time for blacks than for whites. This could account in part for the instability of the veteran status coefficient in the black wage equation.

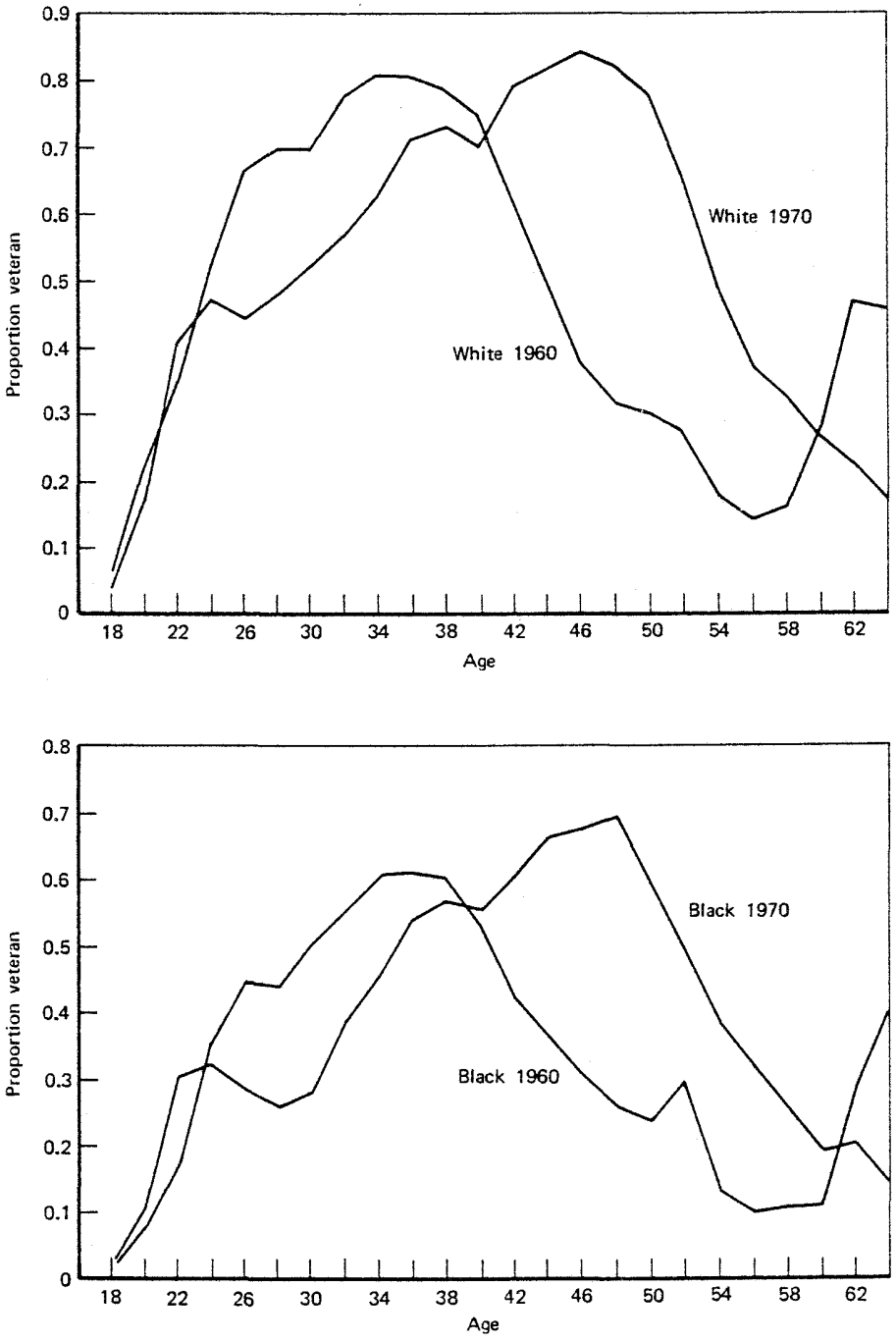

Fig. 1—Proportion of veterans by age

initial earnings but high peak earnings, and by Fig. 1, this will be positively correlated with the proportion veteran in young versus peak earnings age groups.

The alternative hypothesis is that regardless of their proportional representation in the different age groups, veterans will exhibit low initial and high peak earnings when compared with nonveterans because of their differential human capital investment behavior. Results in Table 13 would then merely reflect the inverted U-shaped pattern of proportion veteran across age groups.

A test of this alternative hypothesis can be developed based on the following reasoning. If the results presented above reflect mainly human capital investment behavior, then the premium paid to veterans of a *given cohort* ought to vary predictably between 1960 and 1970. For the sake of illustration, let us focus on men who were aged 30 in 1960, and 40 in 1970. In 1960, these men were at a fairly early point in their age earnings histories and, according to human capital theory, would be investing quite heavily in on-the-job training. Investments in on-the-job training are paid for partly through forgone earnings, and if the more able (veterans, in our analysis) invest more heavily in human capital, their 1960 wages would be depressed relative to less able workers.

A decade later this story changes considerably. This cohort is approaching its peak lifetime earnings phase, characterized by human capital theory as a period of small or no human capital investment activities. Further, more able workers in the cohort (veterans) would be reaping the benefits of their heavy OJT investments made a decade earlier. Both factors will increase the wages received by able workers (veterans) relative to wages of less able workers.

In contrast, the screening hypothesis predicts, at best, that there should be no within-cohort change in the premium associated with veteran status unless a cohort exhibits significant change in the proportion of men claiming veteran status between 1960 and 1970. In fact, one could argue that the value of veteran status as a screen should diminish with age as employers accumulate more discriminating measures of employee ability (for example, their work history).

To test for within-cohort changes in the effect of veteran status on earnings, blacks and whites who were aged 37 or less in 1960 were restratified into two-year age groups.[10] These groups were then pooled into cohort cells across the two census years (ages t in 1960 with ages t+10 in 1970) and regressions estimated in which all coefficients were allowed to vary by census year. The results for the change in veteran coefficient between 1960 and 1970 are given in Table 14 along with the change in the proportion of veterans in each cohort. For age cohorts in which the change in proportion veteran was less than 10 percent, there is no instance of a significant positive increase in the premium associated with veteran status. These findings appear to reject the human capital explanation for the veteran premium.

A peculiarity of this particular data set once again reduces the discriminating power of the preceding test. The year for the 1960 Census income data, 1959, was a year of poor economic conditions and high unemployment rates (4.6 percent for white males and 11.5 percent for black males). In contrast, the business climate in 1969 was good and male unemployment rates were low (2.5 percent for whites, 5.3 percent for blacks). This fact may introduce a bias into the veteran premium comparisons that would work against accepting the human capital investment explanation. If more able workers receive larger doses of firm-specific investment than do less able workers, in times of declining business conditions, firms have an incentive to keep employees in whom they have invested large amounts of firm-specific capital. This incentive implies that unemployment and wage rates of more able persons fluctuate less over the business cycle than these same rates for less able people.[11]

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[10]A four-year age classification produces only four meaningful tests (1960 ages 22-25 to 34-37); however, for completeness, these tests are given in Appendix C.

[11]This statement assumes a positive correlation between unemployment rates and wages *within* skill classes. Patricia Munch, in commenting on an earlier draft of this report, has pointed out that another factor working against the human

Table 14

TEST OF COHORT EQUALITY BETWEEN 1960 AND 1970 VETERAN STATUS COEFFICIENTS

	1960 Age Group (=1970 Age-10)	Coefficient Change	t-ratio on Difference	Change in Proportion Veteran
White	22-23	.093	2.21	.216
	24-25	.110	2.85	.113
	26-27	.061	1.52	.038
	28-29	-.053	-1.34	.035
	30-31	-.029	-.77	.004
	32-33	-.036	-.85	.013
	34-35	.019	.46	.002
	36-37	-.025	-.54	.037
Black	22-23	.047	.77	.211
	24-25	.110	2.18	.108
	26-27	.063	1.33	.088
	28-29	.100	2.00	.129
	30-31	-.0047	-.10	.052
	32-33	-.052	-1.04	.043
	34-35	-.031	-.67	.056
	36-37	-.014	-.28	.070

Because 1959 was a period of high unemployment, a veteran premium measured in that year would tend to overstate the true equilibrium level of the premium. If the converse were true for 1969, veteran premiums measured in that year would understate equilibrium-level premiums, and the reactions of employers to the firm-specific capital held by their employees would counteract the life-cycle effects of the investment hypothesis.

Additional evidence on the screening value of veteran status can be derived by recognizing the "coarse" nature of veteran status as a screening device. An employer interviewing two college graduates may find that veteran status supplies little or no net information on the relative ability of the candidates; for two high school dropouts, however, veteran status may be a quite accurate indicator of relative productivity. This notion translates into the following hypotheses:

- Because the quality of schooling varies more for blacks than for whites, veteran status will be a more useful screen for blacks than for whites.
- Other things equal, the premium to veteran status will diminish as schooling levels rise.

Nonrigorous tests of these hypotheses are given in Figs. 2 and 3. The first figure indicates that for *given* levels of proportion veteran, blacks receive a higher premium than whites for having served in the military with the exception of proportions lying outside the range of the data.

To test the relationship between schooling and veteran premium, the four-year samples were further stratified into two schooling levels, 0-11 and 12+, and the relationship between the veteran premium and proportion veteran was recalculated within each schooling group. These regressions and the plots of the estimated relationships are given in Table 15 and Fig. 3, respectively (the raw data for these calculations can be found in Appendix C).

capital hypothesis is the changing sample base between the two years. If the return to military experience in the civilian sector is less than the return to civilian experience, then the addition to the veteran pool of those who have served lengthy tours of duty in the military (those of a given cohort who were in the military in 1960, but out in 1970) will tend to depress the veteran premium. See, however, Cooper (forthcoming) on this point.

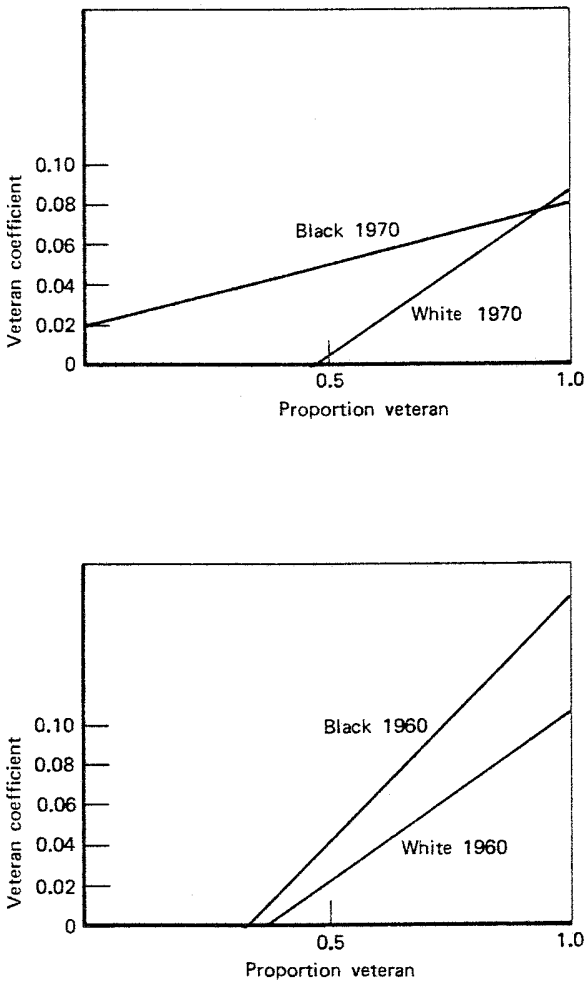

Fig. 2—Veteran premium and proportion veteran: Race comparisons

For all four race/census year groups, veteran status yields higher premiums to men with less than 12 years of schooling than to men with 12 or more years of schooling. The continued positive association between veteran coefficients and proportion veteran for the 12+ groups is also noteworthy in that it suggests that life cycle human capital investment behavior is probably also at work in determining the levels of these relationships.

Rand colleagues have suggested an alternative explanation for these patterns. They argue that as the proportion of a given age cohort who are drafted rises, the average ability of those in the armed services rises. This is so because drafting has never been a random process, with those who face the best civilian opportunities willing to expend resources, sometimes considerable resources, to avoid service in the military. Thus, the relationship between proportion

Table 15

VETERAN COEFFICIENT REGRESSIONS: EDUCATIONAL STRATIFICATION

Race/Education	Explanatory Variables: Percent Veteran	Constant	R^2	F	N
Whites, 1970					
0-11	.10	-.003	0.38	5.4	11
	(2.3)	(-.15)			
12+	.16	-.080	0.34	4.7	11
	(.2)	(-1.8)			
Whites, 1960					
0-11	.14	-.16	0.27	3.3	11
	(1.8)	(2.2)			
12+[a]					
Blacks, 1970					
0.11[a]	.073	.031	0.15	1.65	11
	(1.28)	(1.26)			
12+	.020	.030	0.01	0.10	11
	(.32)	(.60)			
Blacks, 1960					
0.11	.31	-.09	0.67	18.00	11
	(4.2)	(-3.6)			
12+[a]	.19	-.097	0.22	2.60	11
	(1.60)	(-1.06)			

[a]Regression weighted by 1/(s.e. of the veteran status coefficient).

veteran and the veteran premium in Table 15 and Figs. 2 and 3 indicates only that during periods of intensive drafting, for example during wars, men with good civilian alternatives are drafted more frequently than when draft calls are low. When these men get out of the service, they return to their previous civilian activities. Because these activities pay better than average, the veteran status premium rises with proportion veteran.

Two objections can be raised against this explanation. The first is that in times of high draft calls, both more able *and* less able people may be called up in abnormally high proportions; that is, standards may be lowered. The net effect on the veteran premium of drafting more of those with especially good civilian alternatives while lowering entrance standards is ambiguous. The second point is that, to some extent, differences in the types of alternatives that civilians face are controlled for by including education and location variables in the wage regressions. Neither of these counter arguments is completely satisfactory, and the issue is one that should receive careful consideration in future work.

The preceding comparisons suggest that veteran status is valuable as a screening device. Decisions on human capital investment and employer reactions to firm-specific investments in employees are likely also to have played a role in shaping the empirical findings discussed above; but these forces are not, by themselves, sufficient to produce the observed patterns of wage premiums received by veterans of different ages.

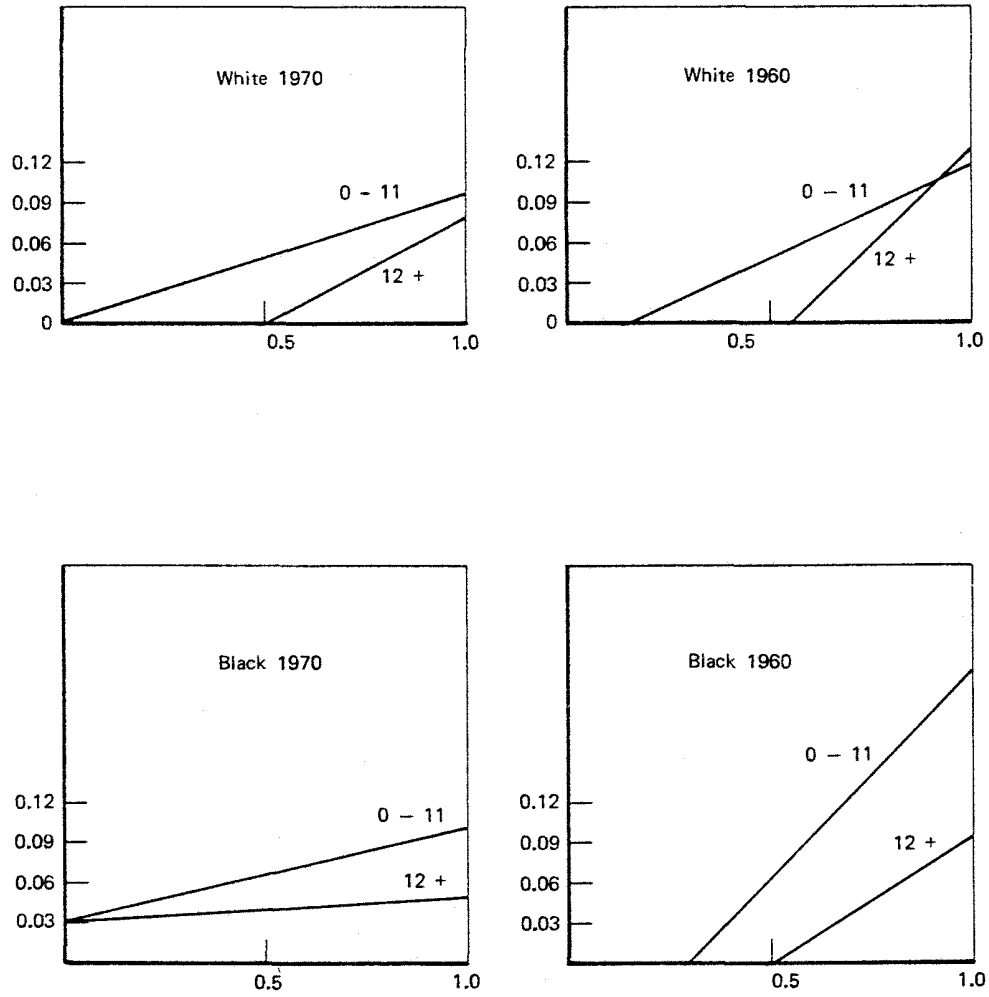

Fig. 3—Veteran premium and proportion veteran:
Educational comparisons

IV. CONCLUSIONS

Statistical analyses of nonexperimental data seldom produce unequivocal results either for or against a given hypothesis. Such is the case here. But when one looks beyond the many individual tests to the overall picture created by the preceding analysis, the following conclusions are justified:

- Veterans earn more than nonveterans of comparable age, labor market experience, education, and vocational training. This result holds across a variety of data sets, age groups, and for both black and white veterans.
- The correct explanation for this premium is likely to involve a combination of innate productivity differences and differences in optimal human capital investment patterns for individuals with greater ability versus individuals with less ability.
- Training received in the military increases civilian wages when innate differences in productivity are dealt with appropriately.

This last point is, perhaps, the most important finding of this research. Those who receive training in the military differ from other veterans and from the population at large in ways not easily measured. And when these innate and difficult to observe characteristics are not appropriately controlled for, a highly misleading picture of the "returns" to military training may result. This report does little more than hint at the outcome of an appropriate analysis of military training; a full-scale analysis must await the release of the final year of the NLS Young Men's Panel and will involve efficient use of the full 10-year time-series component of that survey.

Appendix A

SAMPLE SELECTION

The final working sample for this analysis consists of 1275 white and 427 black observations. These samples were culled from an original data base of 5225 observations that constituted the 1966 panel of National Longitudinal Survey of Young Men. For each of the exclusions leading to the final working samples, the following table provides the number of observations present after the exclusion and the number of cases lost because of that exclusion.

Reason for Exclusion	Sample Size After Exclusion	Cases Lost
Base sample	5225	
Not interviewed in 1971	3987	1238
Race neither white nor black	3946	41
Enrolled in school	3147	799
Not currently working	2712	435
Not interviewed in 1967, 1968, 1969	2584	128
Invalid codes for SMSA, collective bargaining, hourly rate of pay, tenure on current job or years of education	2307	277
Invalid codes for armed forces variables	2215	92
Invalid codes for employment and unemployment variables	1732	483
Inconsistent armed forces information	1702	30

Appendix B

REGRESSION RESULTS: BLACK SAMPLE

Table B.1

MEAN AND STANDARD DEVIATIONS, BLACKS, 1971
(n = 427)

Variable	Mean	Standard Deviation
Wage ($)	2.78	1.30
Education (years)	10.55	2.84
Age (years)	23.75	3.15
Experience (years)	5.50	2.90
Employer experience (years)	2.31	2.26
Binary variables (proportions)		
Vocational training	0.32	
Veteran	0.09	
Disabled	0.07	
Region (South = 1)	0.71	

Table B.2

Basic Wage Equations: Blacks

(Dependent variable; ln(Wage);
n = 427; t-ratios in parentheses)

	Coefficient	
Explanatory Variable	1	2
Education	0.05 (7.46)	0.05 (7.50)
Experience	0.05 (1.78)	0.03 (1.11)
(Experience)^2	†0.002 (-1.45)	-0.002 (-0.91)
Years with current employer		0.03 (1.59)
(Years with current employer)^2		0.002 (-1.06)
Age	0.02 (1.44)	0.01 (1.41)
Disability	-0.11 (-0.70)	-0.11 (-1.64)
Vocational training	0.03 (0.74)	0.03 (0.81)
Veteran	-0.06 (-0.98)	-0.05 (-0.88)
Region (1 = South)	-0.28 (-7.03)	-0.28 (-6.94)
SMSA	0.21 (5.41)	0.20 (5.29)
Intercept	-0.03	-0.02
R^2	0.44	0.45
F	36.86	30.56

Table B.3

Summary Statistics for Military Variables: Blacks

Category	Proportion or Mean	Base
Rejected from military	0.49	427[a]
On physical grounds	0.17	"
On mental grounds	0.06	"
On physical and mental grounds	0.01	"
Other reasons	0.25	"
Veteran	0.09	"
Drafted	0.68	All veterans
Served in army	0.83	
Length of service	22.9	"
Served more than 20 months	0.65	
Received training in service[b]	0.48	"
Length of training	2.84	Vets with training
Type of training		
Professional/technical	0.05	"
Managerial	0.05	"
Clerical/sales	0.16	"
Skilled manual	0.37	"
Other	0.37	"

[a]Black, working males not in school in 1971.

[b]Excludes those who received training classified as "military only."

Table B.4

Effect of Military Variables on Civilian Earnings: Blacks

(t-ratios in parentheses)

Explanatory Variable[a]	Coefficient						
	Eq. 1	Eq. 2	Eq. 3	Eq. 4	Eq. 5	Eq. 6	Eq.7
Veteran	-0.05						
	(-0.88)						
Served six months or less[b]		0.22	0.22	0.22	0.14	0.14	0.14
		(1.18)	(1.16)	(1.06)	(0.74)	(0.72)	(0.72)
Served seven months or more[b]		-0.08	0.09	-0.09	-0.15	-0.12	-0.12
		(-1.27)	(0.34)	(-0.87)	(-2.43)	(-1.49)	(-1.49)
Served seven to 24 months[c]			0.01				
			(-0.72)				
Served 25 to 36 months[c]			0.01				
			(0.89)				
Served 37 months or more[c]			-0.02				
			(-0.87)				
Drafted				0.01			
				(0.08)			
Trained in military						-0.07	-0.22
						(-0.64)	(-1.27)
Months of military training							0.05
							(1.12)
Rejected from military:							
For physical reasons					-0.11	-0.11	-0.11
					(-2.47)	(-2.49)	(-2.48)
For mental reasons					-0.14	-0.14	-0.14
					(-1.92)	(-1.92)	(-1.91)
For physical and mental reasons					-0.20	-0.20	-0.20
					(-1.36)	(-1.36)	(-1.36)
For other reasons					-0.17	-0.17	-0.17
					(-4.19)	(-4.20)	(-4.19)

[a]For other variables included in regression, see Table B.1, Eq. 2.

[b]Binary variable.

[c]Continuous variable.

Table B.5

YEARS SINCE SERVED RESULTS: BLACKS

(t-ratios in parentheses)

Explanatory Variable[a]	Coefficient	
	Eq. 1	Eq. 2
Served six months or less[b]	0.17	0.14
	(0.77)	(0.63)
Served seven months or less[b]	0.09	0.11
	(0.37)	(0.43)
Served seven to 24 months[c]	-0.005	-0.006
	(-0.36)	(-0.41)
Served 25 to 36 months[c]	0.04	0.04
	(1.68)	(1.80)
Served 37 months or more	-0.01	-0.01
	(-0.36)	(-0.35)
Drafted	0.09	0.11
	(0.50)	(0.57)
Trained in military	-0.27	-0.31
	(-1.40)	(-1.21)
Months of military training	0.11	0.10
	(1.97)	(1.86)
Years since served in military	-0.07	-0.08
	(-1.71)	(-1.46)
(Years since served) x (trained)[d]		0.02
		(0.26)

[a]For other variables included in regression, see Eq. 2, Table B.2.

[b]Binary variable.

[c]Continuous variable.

[d]Interaction of "Years since served in military" and "Trained in military."

Appendix C

ADDITIONAL RESULTS: SECTION III

Table C.1

EFFECT OF VETERAN STATUS ON (LN) HOURLY WAGE BY AGE: TWO YEAR AGE GROUPS

Race/Age Group	Veteran Status Coefficient	t-ratio	% Veteran	N
Whites, 1970				
22-23	-0.022	-0.73	0.40	2071
24-25	-0.070	-2.63	0.47	1923
26-27	-0.028	-1.24	0.45	2210
28-29	-0.057	-2.59	0.48	2070
30-31	0.017	0.70	0.53	1845
32-33	0.032	1.31	0.57	1729
34-35	0.044	1.77	0.62	1681
36-37	0.071	2.48	0.71	1615
38-39	0.049	1.68	0.73	1664
40-41	0.020	0.76	0.70	1698
42-43	0.077	2.44	0.79	1774
44-45	0.091	2.99	0.81	1781
46-47	0.037	0.99	0.84	1737
48-49	0.013	0.38	0.82	1774
50-51	0.106	3.40	0.78	1627
52-53	-0.011	-0.40	0.65	1508
54-55	0.011	0.38	0.50	1417
56-57	0.031	1.01	0.37	1338
58-59	0.026	0.73	0.33	1214
60-61	-0.017	-0.40	0.27	1121
62-63	-0.013	-09.27	0.28	860
64-65	0.083	0.97	0.17	583
58-59	0.026	0.73	0.33	1214
60-61	-0.017	-0.40	0.27	1121
62-63	-0.013	-0.27	0.28	860
64-65	0.083	0.97	0.17	583
Whites, 1960				
22-23	-0.061	-1.70	0.36	1310
24-25	-0.065	-2.20	0.51	1446
26-27	0.009	0.32	0.67	1582
28-29	0.102	3.78	0.70	1664
30-31	0.049	1.92	0.70	1728
32-33	0.113	4.09	0.78	1869
34-35	0.072	2.49	0.81	1827
36-37	0.062	2.09	0.81	1736
38-39	0.121	4.12	0.79	1764
40-41	0.078	2.60	0.75	1690
42-43	-0.022	-0.84	0.62	1643
44-45	0.0058	0.24	0.48	1592
46-47	0.030	1.07	0.38	1578
48-49	00.021	-0.69	0.32	1398
50-51	-0.023	-0.70	0.30	1348
52-53	-0.0094	-0.26	0.27	1171
54-55	-0.035	-0.81	0.81	1212
56-57	-0.048	-0.94	0.14	1036
58-59	0.113	2.24	0.17	996
60-61	0.034	0.68	0.27	800
62-63	0.092	1.83	0.46	696
64-65	0.227	4.10	0.45	537

Table C.1—continued

Race/Age Group	Coefficient	t-ratio	% Veteran	N
Blacks, 1970				
22-23	-0.012	-0.30	0.30	1687
24-25	0.043	1.16	0.32	1756
26-27	0.099	0.30	0.28	1745
28-29	0.048	1.48	0.26	1644
30-31	0.036	1.06	0.28	1611
32-33	0.097	2.97	0.39	1430
34-35	0.012	0.39	0.46	1576
36-37	0.062	1.90	0.54	1540
38-39	0.038	1.11	0.57	1457
40-41	0.039	1.22	0.56	1514
42-43	0.039	1.08	0.60	1487
44-45	0.060	1.79	0.66	1523
46-47	0.063	1.76	0.68	1468
48-49	0.123	3.40	0.69	1384
50-51	0.021	0.54	0.59	1368
52-53	0.011	2.98	0.50	1104
54-55	0.0010	0.26	0.39	1082
56-57	0.052	1.25	0.33	1015
58-59	0.045	1.01	0.27	1007
60-61	0.088	1.51	0.19	766
62-63	0.073	1.12	0.21	634
64-65	0.073	0.77	0.14	481
Blacks, 1960				
22-23	0.050	0.94	0.18	1187
24-25	-0.096	-2.40	0.35	1253
26-27	-0.0002	-0.54	0.45	1390
28-29	-0.064	-1.69	0.44	1331
30-31	0.044	1.33	0.50	1421
32-33	0.091	2.70	0.56	1485
34-35	0.091	2.91	0.61	1550
36-37	0.076	2.30	0.61	1541
38-39	0.053	1.67	0.61	1466
40-41	0.040	1.25	0.53	1448
42-43	0.050	1.33	0.43	1282
44-45	0.0096	0.25	0.37	1251
46-47	0.0029	0.69	0.31	1198
48-49	0.044	1.07	0.27	1256
50-51	0.022	0.44	0.25	1105
52-53	-0.067	-1.29	0.21	964
54-55	-0.131	-2.03	0.14	952
56-57	0.012	0.16	0.11	773
58-59	-0.056	-0.73	0.12	805
60-61	0.031	0.35	0.12	533
62-63	0.053	0.65	0.30	422
64-65	-0.066	-0.84	0.40	376

Table C.2

VETERAN COEFFICIENT REGRESSIONS: TWO-YEAR AGE GROUPS
(t-ratios in parentheses)

Race/Age Category	Explanatory Variables: Percent Veteran	Intercept	R^2	F	N
Whites, 1970					
22-65	0.175	-.085	.37	11.80	22
	(3.44)				
Whites, 1960					
22-65	0.166	-.058	.30	8.08	22
	(2.95)				
Blacks, 1970					
22-65	0.146	-.048	.19	4.78	22
	(2.19)				
Blacks, 1960	0.176	-.054	.23	6.04	22
	(2.46)				

Table C.3

TEST OF COHORT EQUALITY: FOUR-YEAR AGE GROUPS

		Age Group	Coefficient Change	t-ratio on Difference	Change in Proportion Veteran
A.	Whites, 1960	22-25	.0990	3.530	0.16
		26-29	.0037	0.130	0.03
		30-33	-.0340	-1.240	0.06
		34-37	.0011	0.035	0.02
B.	Blacks 1960	22-25	.089	2.32	0.15
		26-29	.077	2.24	0.11
		30-33	-.028	-0.81	0.05
		34-37	-.026	-0.78	0.06

Table C.4

Effect of Veteran Status on (ln) Hourly Wage by Age, Schooling, and Proportion Veteran

Race/Age Group	0-11 Years of Schooling				12+ Years of Schooling			
	Veteran Coefficient	t	Proportion Veteran	N	Veteran Coefficient	t	Proportion Veteran	N
Whites, 1970								
22-25	.0623	1.37	.372	817	.0835	-3.4	.45	3177
26-29	.0187	.46	.345	963	-.07	-3.9	.497	3317
30-33	.0968	2.5	.416	949	.0103	.53	.596	2625
34-37	.0884	2.63	.565	982	.0562	2.46	.71	2314
38-41	.0429	1.3	.614	1183	.0462	1.86	.77	2179
42-45	.0981	3.12	.738	1449	.0983	3.2	.845	2106
46-49	.0817	2.35	.763	1418	-.0033	-.9	.878	2093
50-53	.0322	1.13	.549	1395	.0674	2.22	.772	1740
54-57	.0362	1.3	.391	1391	.0068	.23	.485	1364
58-61	.0102	.27	.254	1349	.0091	.23	.36	986
62-65	.0193	.33	.174	880	-.0038	-.56	.258	563
Whites, 1960								
22-25	-.0355	-.89	.394	950	-.0844	-2.9	.461	1806
26-29	.1303	3.71	.571	1126	-.013	-.54	.746	2120
30-33	.0803	2.72	.663	1478	.0815	3.33	.79	2119
34-37	.0566	1.94	.746	1569	.0864	2.88	.856	1994
38-41	.0626	2.04	.706	1550	.1512	5.18	.817	1904
42-45	-.0014	-.56	.506	1619	-.0157	-.62	.601	1616
46-49	.0485	1.85	.306	1772	-.0568	-1.7	.409	1204
50-53	-.0327	-1.0	.262	1609	.0089	.22	.329	910
54-57	.011	.28	.151	1548	-.1429	-2.4	.183	700
62-65	.1122	2.69	.391	931	.2564	3.2	.659	302
Blacks, 1970								
22-25	-.068	-1.1	.14	1350	.0361	1.13	.422	2093
26-29	.0944	1.85	.113	1475	-.0061	-.24	.399	1914
30-33	.0493	1.22	.197	1523	.0829	2.92	.468	1518
34-37	.0499	1.63	.356	1776	.0043	.13	.683	1340
38-41	.0692	2.32	.429	1824	-.0265	-.69	.776	1147
42-45	.0689	2.39	.55	2033	.0438	.92	.808	977
46-49	.082	2.81	.633	2038	.1256	2.51	.826	814
50-53	.0725	2.31	.498	1881	.0359	.66	.723	591
54-57	.0207	.64	.33	1692	.0707	1.32	.472	405
58-61	.0766	1.88	.209	1483	.0498	.73	.359	290
62-65	.0698	1.14	.16	971	.0185	.17	.313	144
Blacks, 1960								
22-25	-.0345	-.76	.196	1559	-.0527	-1.1	.392	881
26-29	-.0373	-1.1	.34	1843	-.0155	-.43	.313	144
30-33	.0822	2.86	.451	2047	.05	1.22	.728	859
34-37	.0927	3.43	.551	2274	.0587	1.45	.774	817
38-41	.0414	1.53	.513	2191	.0805	2.03	.741	723
42-45	.0197	.64	.363	2063	.0877	1.85	.572	470
46-49	.0444	1.35	.264	2088	-.0638	-1.0	.423	366
50-53	.0193	.49	.206	1805	-.1651	-2.2	.386	264
54-57	-.0892	-1.6	.114	1530	.0241	.28	.215	195
58-61	-.0581	-.9	.109	1208	.1651	1.5	.192	130
62-65	-.0031	-.52	.335	713	-.0875	-.52	.459	85

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